

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC
PROGRESS.

JOHN MICHELS, Editor.

TERMS:

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PUBLISHED AT

TRIBUNE BUILDING, NEW YORK.

P. O. Box 3838.

SATURDAY, SEPTEMBER 24, 1881.

ENCKE'S COMET.

This comet is now visible in telescopes of moderate power, and will increase in brightness until November, when it may be visible to the naked eye. The corrections to the ephemeris, computed by Dr. Backlund, of the Pulkowa Observatory, are as follows:

$$\Delta a = -39^{\circ}.0 : \Delta \delta = -1'.4$$

These corrections may vary a little as the comet approaches the earth, but it can be found without trouble. If we consider the great care and labor that have been given to the calculation of the ephemeris; and the fact that the perturbations by nearly all the principal planets have been computed, as well as the effect of the resisting medium in space, the corrections to the ephemeris seem to be very large. This comet affords another example of what is now most needed in Astronomy, viz., complete and careful theoretical investigations. It will be comparatively easy to obtain a great number of observations of this comet during its present return, while a much smaller number of good observations is sufficient. The attention of astronomers should be given rather to a satisfactory determination of the motion of the comet, since the recent computers of its orbit do not have the success of Encke in predicting its returns.

THE WARNER-ASTRONOMICAL PRIZES.

We recently explained, in an editorial, the conditions on which Mr. H. H. Warner consents to present to each of the discoverers of comets during the year 1881, the sum of two hundred dollars. We also stated that applicants for the prize for Comet *b*, 1881, were presenting their claims at the rate of sixty *per diem*.

We now learn by a communication from Mr. C. S. Whittemore, secretary to the Rochester Astronomical

Society, that nearly 3000 letters were received claiming priority in the discovery of this comet, all of which have been examined. As a result of such examination, Professor Lewis Swift reports that "no conclusion can be reached that would be scientific and satisfactory." In other words, the claims of the 3000 applicants are ignored, and the prize of \$200 for this, the most important of the three comets, so far discovered in 1881, is withdrawn.

We cannot refrain from expressing our dissatisfaction with this decision, and the methods employed in arriving at it, which we believe to be neither "scientific nor satisfactory."

Mr. Warner, in a letter to the public dated September 5th, states, that two of the conditions on which he consented to give a prize of \$200 to the discoverer of every comet appearing in 1881, were as follows: That it should be "telescopic" and "unexpected." He now claims that "Comet *b* was neither telescopic nor unexpected." Under these circumstances if Mr. Warner had simply announced that Comet *b* did not come within the meaning of his advertised prize, his course would have been intelligible and satisfactory. But he himself has stated, that in spite of these disqualifying circumstances, he "was anxious, could the first discoverer be found, to make a special reward of \$200." In other words, a decision was arrived at to waive the disqualifications, and to proceed as if they did not exist, and the same letter admits that Professor Lewis Swift "examined" the 3000 applications on their merits. Such being the case, when Professor Swift found that he was unable to arrive at a "scientific and satisfactory conclusion," he should (under the terms of the contract between Mr. Warner and the public), have instantly referred the matter to Professor Asaph Hall, of Washington, and the other gentleman named as referee.

Under these circumstances we consider that Mr. Warner is under the moral obligation of carrying out his contract in regard to this matter, and insisting on Professor Swift taking the proper steps to arrive at some decision. Probably the mere perusal of the 3000 letters would instantly reduce the number to some half dozen applicants, whose cases could be submitted to Professor Hall, who would probably decide upon their merits within a week.

The second point in Mr. Warner's letter to which we would draw attention, is that in which he states, that to mitigate his disappointment in not being able to trace the "first discoverer" of Comet *b*, he proposes as a balm to the claimants, and to encourage astronomical study, to offer a prize of \$200 to the person who shall prepare the best essay on "COMETS: *Their Composition, Purpose and Effect upon the Earth.*"

The conditions are as follows :

"1st. The essay must be written in plain language, each term to be defined in brackets immediately following, and must not exceed 3000 words.

"2nd. Each essay must be signed with a *nom de plume*, and a sealed envelope must accompany the essay superscribed with the *nom de plume*, and containing the real name of the author.

"3rd. All the essays must be filed with Dr. Lewis Swift, Director of the Warner Observatory, Rochester, N. Y., by November 1, 1881, and he will submit them to the judges.

"I hope that this prize will produce valuable additions to popular astronomical literature."

We regret that Mr. Warner was not better advised when he arranged the title of the essay and named the conditions. By "*plain*" language we presume he calls for "simple" language, but we are unable to explain his desire that "each technical term shall be defined in brackets immediately following." For whose benefit is all this defining to be offered? Is it to aid Professor Swift and the judges? If to aid readers, when the essay is subsequently published, would not a simple glossary of the scientific terms used, added at the end, be more appropriate? If "*each* technical term is to be defined," we fear that a large percentage of the 3000 words permitted will be used for this purpose.

Again, would it not be a more creditable arrangement, that the essays be filed with some independent person, instead of Professor Swift, who is at least a beneficiary of Mr. Warner, and is both a competitor and the judge in these prize gifts? (We think Professor Swift awarded the first Warner prize for comets to himself.)

Lastly, we find that no names are given of those who are to be judges of the value of the prize essays. This omission is very important, and seems to raise a doubt whether any judges whose opinion is worthy of respect can be secured to connect themselves with a scheme proposed under such conditions. Again, what disposition is to be made of the essays received by Mr. Warner? nothing is guaranteed in this respect; and will Professor Swift once more announce to the essayists that "no conclusion can be reached which is scientific and satisfactory?"

We do not wish our remarks to be interpreted in a sense which implies that either Mr. Warner or Professor Swift are desirous of acting improperly in this matter, although their behaviour may, in some quarters, be severely criticised; we rather lean to the view that their judgment and discretion is at fault, and that they require the counsel of some friend who can so advise them, that they arrive at "conclusions which

are scientific and satisfactory." Mr. Warner hopes that his prize "will produce valuable additions to popular astronomical literature;" we fear that under the conditions he offers, he will be inundated with vulgar scientific trash.

In conclusion, we offer Mr. Warner one word of advice. If he honestly desires to encourage real scientific work and literature, let him permit such men as Hall, Newcomb, Pickering, Young, Stone, Holder or Draper to arrange the title of his prize astronomical essay, and request them to name the conditions, and be the judges, of the merits of the papers submitted. The decision of any two of the gentlemen we have named would be satisfactory to those who are likely to be competitors, provided they acted independently, and untrammelled by Mr. Warner or any of his Rochester friends.

HYPERMNESIA OR EXALTATIONS OF MEMORY.*

[Translated from the French by the Marchioness CLARA LANZA.]

Until now our pathological study has been confined to destructive forms of memory. We have seen the latter diminished, sometimes completely destroyed. There are however, precisely contrary cases, in which the apparently abolished memory comes to life again as it were, and faint recollections become intensely vivid. Is this exaltation of the memory (called technically hypermnesia) a morbid phenomena? It is at least certainly an anomaly. When we remark further that it is always connected with some organic disorder or bizarre condition, we cannot deny that it comes within our province to discuss it. There are other subjects, amnesia for instance, which are more instructive, but we should not neglect it for that reason. We will see therefore what there is to learn about *persistence of recollections*.

Hypermnesia is divided into two classes—general and partial.

General exaltation of memory is difficult to determine, because the degree of excitation is quite relative. The force of this faculty varying to a great extent in different individuals we cannot measure it by any common standard. The amnesia of one person may possibly be the hypermnesia of another. It is, if we may employ the word, a change of *tone* in the memory, such as occurs in every other form of psychological activity, thought, imagination or sensibility. Moreover, when we say that the excitation is general, it is nothing more than a probable induction. Inasmuch as the memory is subject to the condition of our consciousness, and as consciousness is only produced in the form of succession, all that we can affirm is, that in the course of a period more or less extended a mass of recollections spring up in every direction.

General exaltation seems to depend exclusively upon physiological causes, particularly the rapidity of cerebral circulation. It is therefore apparent very often, in cases of acute fever. It is also produced in insanity, ecstasy, and hypnotism, sometimes in hysteria and in the beginning of certain mental diseases.

Besides these purely mental pathological instances there are others of a more wonderful nature which depend probably upon the same cause. Numbers of persons who narrowly escaped drowning have stated, that in the moment when asphyxia began, they seemed to see all at once their entire life in all its details, even the most

* See *Les Maladies de la Mémoire* by Th. Ribot, Paris 1881.

trifling incidents appear distinctly before them. One person in particular, declared that he saw his whole existence rolled out in retrograde succession, not like a mere indistinct sketch, but in precise details, making a panorama of his life, in which every act was accompanied by a feeling of pleasure or pain.

An analogous circumstance relates to a man of remarkable intelligence who happened to cross a railroad track just as an express train approached. He had just time to throw himself lengthwise between the rails. As the train passed over him, the sense of his danger caused every incident in his life to suddenly rise before him in memory.

Even allowing for possible exaggerations, these facts reveal a hyper-activity of the memory, of which in a normal state we can form no idea.

I will quote one more instance, due to stupefaction from opium, and at the same time I will beg the reader to observe how well it confirmed the explanation of the mechanism of recollection given in another chapter.

"It seems to me," says DeQuincy in his celebrated *Confessions*, "that I have lived seventy years or a whole century in one night. The most trivial events of my youth, forgotten scenes belonging to my early years, were constantly brought before me. It cannot be said that I remembered them, for had they been mentioned to me while awake, I should not have been capable of recalling a single one of them as forming a part of my previous existence. But placed before me as they were in a dream, like so many intuitions, made up of the most vague circumstances and their accompanying sentiments, I recognized them instantly."

All these general excitations are transitory. They endure no longer than the causes which produce them. Does permanent hypermnnesia exist? If the word can be accepted in this rather forced sense, it must be applied to those singular developments of the memory which are the continuation of some chance accident. In ancient authors we find many cases of this kind related. We have no reason to doubt them, for modern investigators, Romberg among others, have noted a wonderful and permanent development of the memory, following small-pox, etc. The mechanism of this transformation being impenetrable, we have, however, no reason to insist upon it.

Partial exaltations are by their very nature limited. The ordinary tone of the memory being maintained in its generality, everything beyond this can be easily ascertained. These forms of hypermnnesia are the necessary co-relatives of partial amnesia.

In the production of partial hypermnnesia there is nothing resembling a fixed law. It presents itself under the form of isolated facts, that is to say, it is the result of a series of conditions which escape us. Why is a certain group of cells forming a particular dynamic association put in motion sooner than another? We can give no reason, neither a physiological nor a psychological one. The only cases wherein we can affirm the appearance of a fixed law, are those of which we shall speak later, where several languages return successively to the memory.

Partial exaltations generally spring from morbid causes. But sometimes they occur in a healthy state. Here are two examples:

"A lady in the last stages of a chronic malady was taken from London into the country. Her little daughter, who was a baby, not yet able to speak, was brought with her, and after a short interview, was taken back to the city. A few days later the lady died. The child grew up without having any recollection of her mother. When she had attained maturity, however, she had occasion to see the room in which her mother died. Although she was ignorant of this fact, she trembled as she crossed the threshold. On being asked the cause of her emotion, she replied: 'I have a distinct impression of having been in

this room before. There was a lady in bed here in this corner. She seemed to be very ill, and she bent over me, weeping.'"¹

"A man endowed with a highly artistic temperament (observe this point) accompanied some friends to a castle somewhere in Sussex. He had no recollection of ever having been there before. On approaching the entrance he had a sudden vivid impression of having already seen it, and, with the remembrance of the door came also a recollection of people above and beneath the portico, together with some donkeys standing by. As this conviction grew upon him more and more, he questioned his mother, thinking she, perhaps, might be able to enlighten him. She told him that when he was about sixteen months old he had been in the neighborhood with a large party of people, that he had been carried in a basket upon a donkey to the castle and left down stairs with the servants and their donkeys while the other members of the party had installed themselves above the portico to eat their dinner."²

The mechanism of recollection in these two cases does not admit of any ambiguity. It is a revival, by contiguity, after a long interval. They present that which happens throughout every instant of life in a striking and uncommon form. In order to recover a lost recollection have not many of us returned to the spot where the idea arose, endeavored to place ourselves as nearly as possible in the same material situation and thus see the remembrance spring to life?

As to hypermnnesia arising from a morbid cause I will give one example.

"At the age of four years, a child submitted to trepanning in consequence of fracturing its cranium. When his health was quite restored he had no recollection of the accident or the operation. But at the age of fifteen, while suffering from delirious fever, he described the operation to his mother, mentioning the physicians and those present, the details of their dress, and other minute particulars, with the utmost exactitude. Up to this time he had never spoken of the matter nor had he heard any one mention it."³

The revival of languages completely forgotten deserves to be spoken of more at length. The instance reported by Coleridge is so hackneyed that I shall not repeat it. There are many others of the same kind which are to be found in the writings of Abercrombie, Hamilton and Carpenter. Anæsthetic sleep produced by chloroform or ether can produce the same effects as febrile excitation. "An old forester lived during his youth on the frontier of Poland, and spoke scarcely anything but Polish. Later he removed to German territories. His children testified that for thirty to forty years he had neither heard nor pronounced a single Polish word. During an anæsthetic sleep of about two hours this man spoke, prayed and sang fluently in Polish."⁴

A still more curious thing is the *regressive* recollection of several languages. Unfortunately, the authors who have mentioned this use the term and note the fact without properly interpreting either.

The most interesting case was observed by Dr. Rush, of Philadelphia. An Italian named Dr. Scandella, a man of remarkable learning, resided in America. He was a complete master of the Italian, English and French languages. He took the yellow fever, and died of it in New York. In the beginning of his illness he spoke nothing but English. After that only French, and on the day of his death, Italian—his native language.

The same writer speaks in rather confused terms of a woman who was subject to transitory attacks of acute mania. When first seized she always spoke very poor

¹ Abercrombie. "Essay on Intellectual Powers."

² Carpenter. *Mental Physiology*.

³ Abercrombie. Work before quoted.

⁴ M. Duval. *HYPNOTISME dans le Nouveau dict de Médecine*, p. 144.

Italian. When at the height of her insanity—French. During the first period of recovery, German; and while convalescent, her mother tongue, English.

If we throw aside this regression as far as it deals with several languages and content ourselves with more simple cases, we find abundant and very precise instances. A Frenchman living in England, and speaking English perfectly, received a violent blow upon the head. During his entire illness, he was unable to say a word in any language except his native French.

There is nothing, however, more instructive than the following fact, related also by Dr. Rush: "A Lutheran clergyman, of German origin, living in America, had in his congregation a great number of Germans and Swedes, who, before dying, repeated prayers in their mother tongue. Most of these, he said, he was sure had not spoken a word of German or Swedish for fifty or sixty years."

Winslow has stated that numbers of Catholics, converted to Protestantism, have prayed strictly in accordance with the Roman ritual, during attacks of delirium preceding death.

This revival of languages, lost forms and ceremonies, seems to me only to be interpreted by a particular case of the law of regression. In consequence of a morbid action, which generally precedes death, the most recent impressions of the memory are destroyed, and this annihilation descends gradually until it reaches the oldest acquisitions and impressions. They acquire a temporary activity, and are produced in consciousness for a certain period before being wiped out forever. Hypermnnesia, then, is merely the result of negative conditions. Regression does not follow from a normal return to consciousness, but from the suppression of more intense and vivid conditions. It resembles a weak, faltering voice, which cannot make itself heard until the loud speech of others has ceased. Impressions, certain habits belonging to childhood or youth, suddenly return, not because they are pushed forward by some cause, but because there is no longer anything to cover them. Recollections of this kind, are, strictly speaking, only a march backwards towards certain conditions of existence which seemed to have disappeared forever, but which a final working of the memory, before entire dissolution, brings once more to the surface. I will abstain, however, from further reflections which these facts naturally suggest, reserving them more properly to moralists. It could be shown, for example, that certain religious frenzies, which have overtaken people upon their death-bed, are, to psychology, merely the necessary effect of a general breaking up, for which there is no remedy.

Independent of this confirmation of our regressive law, is the surprising persistence of these latent conditions of memory which we have called residuum. Were it not for these disorders of the memory we could form no idea of it, for consciousness reduced to itself can only affirm the conservation of those conditions which constitute life and a few others dependant upon the will, inasmuch as they have become fixed by habit.

Must we draw, then, the conclusion that nothing is ever lost in the memory? Must we infer that an impression once formed there is indestructible, and that at any moment it is likely to be revived? Several writers, Maury particularly, have given striking examples to uphold this theory. However, there is no peremptory reason to deny that even without the existence of morbid causes, there are residuums which disappear. It is quite possible that certain cellular modifications and other dynamic associations are too unstable to last. In short we may say that the persistence agrees with a fixed rule, at least, in the majority of cases.

As to the manner in which these distant impressions

are preserved and reproduced in memory, we do not know. I will tell you, however, how it can be conceived in the hypothesis which I have adopted throughout this work.

If we admit cellular modifications and dynamic associations to be the material substratum of recollection, there is no memory, however crowded with facts and impressions, which is unable to retain everything. For, if the cellular modifications are limited, the possible dynamic associations are innumerable. We may suppose that the old associations reappear when the new ones, being temporarily or effectually unorganized, leave the road free. The number of possible revivals being diminished the chances augment in proportion for the return of the most stable associations, that is to say, the oldest. I have no desire to insist upon a non-verifiable hypothesis. My aim is to keep closely within the bounds of certainty, and not wander off into doubtful paths.

It has been found impossible to place under the category of any of the preceding morbid types, a certain illusion of a peculiar nature, which occurs rarely, or rather, is seldom observed. There have only been three or four cases mentioned, and up to the present time no special term has been used to designate it. Wigan has called it very improperly, double consciousness. Sander, an illusion of the memory (*Erinnerungs-tauchung*). Other writers have termed it false memory. This latter name seems to me the most preferable. The condition consists in the belief that an entirely new state has been experienced before, so that, when it is produced in reality, for the first time, it seems to be a repetition.

Wigan, in his well known work upon the duality of the mind, says that while present at the funeral service of the Princess Charlotte in Windsor Chapel, he suddenly experienced the impression of having witnessed precisely the same spectacle sometime previous. The illusion was, however, but transitory. Many others of a more lasting nature have been recorded. Lewes associates the phenomenon with some which are more common. When we are in a strange country, for instance, it frequently happens that a sudden turn in a path or river brings us face to face with a view which we are certain we have contemplated before. Sometimes, on being introduced to a stranger, we feel sure that we have already seen him. While reading, new thoughts will often present themselves to the mind as being familiar⁶.

This illusion, I think, can be easily explained. The impression received, evokes in our past, similar or analogous impressions, vague, confused and hardly perceptible, but nevertheless sufficiently defined to make us think the new condition is only a repetition of a former one. There is a basis of resemblance rapidly felt between two conditions of consciousness, which causes one to be identified with the other. It is, of course, an error, but it is only a partial one, because there really exists in the past, something which resembles an identical former state.

If, however, this explanation suffices for simple instances, here are others which do not admit of it.

A sick man, says Sander, on being told of the death of an acquaintance, was seized with an access of ungovernable terror because he thought he had already experienced the impression at some former time. "It seemed to me," he said afterwards, "that I was in the same bed on another occasion and X came to me, saying 'Müller is dead.' I answered, 'he died long ago. How can he die twice?'"

Dr. Arnold Pick reports the most complete case of false memory that I know of. It assumed the form of a chronic disorder. An educated man, of good reasoning powers, was suddenly attacked—about the age of thirty-two—with a most peculiar mental affection. If he was at an entertainment of any sort, or paying a visit, the

⁶ See an article by M. Delboeuf in the *Revue Philosophique*, February 1880.

⁶ Lewes, See *Problems of Life and Mind*, 3d series.

⁷ Sander, *Archiv für Psychiatrie*, 1873, IV.

event, with all its attendant circumstances, appeared so familiar to him that he was absolutely sure he had previously experienced the same impressions, surrounded by the same people or objects, with the same sky, weather, etc. If he ventured to undertake a new occupation of any kind whatever, he was certain he had done it before and under identical conditions. Sometimes the sensation would occur the same day, in the course of a few minutes or hours, sometimes it did not strike him until the day following, but it was always a distinct impression.

In this phenomenon of false memory, there is an anomaly of mental mechanism which escapes us, and which is difficult to comprehend in a healthy state. The person affected, no matter how acute an observer he might be, could only analyze the condition when he ceased to be deceived by it. From the examples given it would seem to me that the impression received is reproduced in the form of some image. To employ a physiological term, there is a repetition of the primitive cerebral process. This is a very ordinary phenomenon. It occurs in every recollection which is not caused by the actual presence of the object. The only difficulty is to discover why this image which arises a minute, an hour or a day after the real condition, should appear to be a repetition of the latter. We may possibly admit that the mechanism of recollection acts in a distorted manner, for my part, however, the following explanation seems more explicit:

The image formed in this manner is very intense and partakes of the nature of an hallucination. It is, apparently, a reality, for nothing rectifies the illusion. Consequently, the real impression is forced back, as it were, and assumes the character of a recollection. It becomes realized in the past, erroneously if we consider the facts, objectively, properly, if we consider them subjectively. The hallucination, although very vivid, does not efface the actual impression, but as the latter is quite separate, and as the former is produced at a comparatively late period, the real occurrence appears to be a second experience. The hallucination assumes the place of the actual impression, it seems to be more recent, and this is really the case. Of course, to us who judge according to what we see externally, it is false to say that the impression was received twice. To the person afflicted, however, who determines solely as his consciousness may dictate, it is true that the impression was actually received twice.

To the support of this explanation, I would call attention to the fact that false memory is nearly always allied to some mental affection. The person mentioned by Pick suffered from a form of insanity. He was continually endeavoring to escape from people he supposed were his persecutors. Hallucinations in this instance would be perfectly natural. I do not, however, wish to assert that my theory is the only possible one. In regard to this isolated condition of false memory, much more numerous and concise observations than mine are probably required.

THE EXCAVATION OF THE GRAND CAÑON OF THE COLORADO RIVER.*

By CAPT. C. E. DUTTON, U. S. A. U. S. Geological Survey.

The Grand Cañon of the Colorado River is the longest, widest and deepest of the almost continuous chain of cañon valleys through which the upper half of that river flows. Its length is 218 miles, its width from 5 to 11 miles, and its depth from 4500 to 6000 feet. For convenience of discussion it may be arbitrarily divided into four divisions: 1st. The Kaibab division; 2d. The Kanab; 3d. The Uinkaret; 4th. The Sheavwits division. The upper or Kaibab division is the grandest, widest and most diversified, and a little deeper than the others

The three others are simpler in form and much alike in their topographical features. Capt. Dutton first exhibited a view of the cañon in the Uinkaret division, showing its simplest and most typical form. It consists of an inner and an outer chasm, or a cañon within a cañon. The outer chasm is five to six miles wide, and is walled on either side with palisades 2000 feet high, of singularly noble and graceful profiles, which confront each other across a comparatively smooth plain. Within this plain is sunken the inner gorge, descending 3000 feet lower, and having a width a little greater than its depth. At the bottom of the inner gorge flows the Colorado River, a stream about as large as the Ohio between Pittsburg and Wheeling. The strata in which the chasm is cut are chiefly of carboniferous age. The summit of the outer cañon wall is very near the summit of that series. The chasm throughout the greater part of its extent cuts below the carboniferous and penetrates the Lower Silurian, and even the Archæan schists, revealing the fact that before the carboniferous was deposited the country had been extensively ravaged by an erosion which swept away heavy bodies of Silurian, and probably also of Devonian strata. The carboniferous now rests upon the beveled edges of the flexed older strata, and in many places rests upon the completely denuded Archæan.

The region adjoining the chasm and for 40 to 60 miles on either side is a nearly level platform presenting the summit beds of the carboniferous system patched over here and there with fading remnants of the Permian. The strata is very nearly, but not quite horizontal. There is a slight dip to the northward rarely exceeding one degree, but as the general course of the river is along the strike, the edges of the strata disclosed in the Cañon walls are to all appearances rigorously horizontal.

From 40 to 60 miles north of the river are found the principal masses of the later formations, including the Permian, Trias, Jurassic, Cretaceous and Lower Eocene. These form a series of terraces rising successively like the steps of a gigantic stairway as we move northward. Each formation is terminated southwardly by a great cliff and the strata are nearly horizontal, collectively they have been named the Southern Terraces of the High Plateaus. The latest formation which was deposited in this region was the Lower Eocene.

To the geologist it is obvious that the formations of the Terraces now terminated by gigantic cliffs once extended further out towards the southward and formerly covered regions from which they have been denuded. Captain Dutton is confident that all these terrace formations once reached entirely across the Grand Cañon platform in full volume, and that their ancient shore line is found in Central Arizona. The thickness of the strata thus denuded was a little more than 10,000 feet on an average, and the area from which they have been swept away is more than 13,000 square miles. It is through the heart of this denuded region that the course of the Grand Cañon is laid. The denudation began probably at an epoch not far from Middle Eocene time, since at that epoch took place the final emergence of the region from a marine condition (through the brackish water and lacustrine stages) to the condition of *terra firma*.

It is apparent that the cutting and development of the present Grand Cañon is only a closing episode of a long history of erosion, extending from Middle Eocene time down to the present. Before the river could begin its attack upon the summit beds of the carboniferous which now form the crests of its upper walls, it had to cut through more than 10,000 feet of superior strata. This would alone indicate that the beginning of the present cañon cannot date far back in Tertiary time, and Capt. Dutton thinks that the evidence points strongly to the conclusion that its excavation in the carboniferous began in Pliocene time. This evidence is cumulative and not direct, but is derived from a comparison of many groups

* Read before the A. A. A. S. Cincinnati, 1881.

of facts which are too numerous and complex to be summarized very briefly. One group of facts bearing upon the question of age is found in the comparative study of the lateral drainage channels and their gradual extinction by the progressive development of the arid climate of the region which took place in Pliocene time. Nearly all the ancient tributaries of the Grand Cañon appear to have dried up at the beginning of its excavation or very soon after, and the whole work shows the influences upon arid climate.

The Grand Cañon district has also been subject to a great amount of uplifting, amounting in the aggregate according to locality, to 16,000 to 19,000 feet. The present elevation of its surface above the sea is the difference between the amount of uplift and the thickness of strata removed, and is from 7,000 to 9,000 feet. This great elevation is considerably surpassed in some other portions of the West. Obviously, it has been an important factor or essential condition in the process of cañon cutting.

The peculiar forms of the drainage channels of the Plateau country, and of which the chasms of the Colorado are extreme developments, are ascribed to the operation of two groups of processes acting under abnormal conditions. It is customary to say that the rivers have cut their cañons. This is but a partial truth, for the rivers cut passages no wider than their water surfaces. The first group of processes is termed corrasion, the result of which is the continuous sinking of the bed of the stream by the grinding action of flowing water charged with sand. Many factors enter into this result, and their mutual relations are highly complex. But in a general way it may be said that a river with a rapid descent, carrying a notable quantity of sediment, but not enough to overload it or overtax its transporting power, will continuously corrade or grind down and deepen its channel. If it is overloaded, a portion of its sediment will be deposited and form a protective covering to the bed-rock. Under special conditions it will actually build up its bed. Most rivers, along their middle and lower courses have their general conditions so adjusted that there is little or no tendency either to build up or corrade. To this equilibrium of adjustment all rivers are tending, and most rivers have nearly or quite reached it. The Colorado is exceptional in this respect, and its tendency is to corrade. Its waters, though carrying great quantities of sediment, are still under-loaded, and could carry more if they could get it. This tendency to corrade may be ascribed to the fact that the country through which it flows has been gradually rising in altitude through Tertiary and probably also Quaternary time, and this elevation produces and maintains a rapid declivity in the stream-bed, which in turn imparts a high velocity, and consequently great transporting power to its waters.

The widening of the cuts made by corrasion is the work of the second group of processes, viz., weathering. This is also a very complex action, and cannot be briefly summarized. To this action is due the remarkable sculpture of the cañon and cliff walls and all those surprising resemblances to architectural forms which are so abundantly displayed in the Plateau country, and most especially in the Grand Cañon.

The concluding portion of Captain Dutton's lecture was devoted to a description of the scenery in the Kaibab division of the cañon, which is declared by all who have seen it to be the most sublime and impressive spectacle in the world.

NEW OBSERVATORY.—A meteorological station is to be erected at Pavia, under the direction of Professor Cantoni. Investigations will be made at this station on the influence of heat, light, electricity, etc., on vegetation in general, and some cultivations in particular, and also the diurnal and annual variations of terrestrial magnetism.

MIXED SUGARS.*

BY PROFESSOR H. W. WILEY.

Mixed sugars are made of cane sugar and *amylose* (starchy sugar.) Within a few years the mixed sugar industry has advanced from a small beginning to a business of considerable importance. It is difficult to get accurate data of the amounts of this sugar made. Manufacturers and dealers are extremely reticent on the whole subject, and often refuse to talk about it at all. I have, however, after considerable trouble, been able to get at the figures which will give at least an approximate estimate.

The principle centers of the grape sugar industry are Brooklyn and New York, Buffalo and Peoria. From a careful comparison of the data which I have been able to collect, I place the daily product of mixed sugars at the several factories at 1,500 barrels. This will be found not far from the truth. It is rather under than over the true number. It is thus seen that the mixing of sugars is a fact which is altogether too large to be laughed at. It must be remembered, too, that the manufacture is rapidly increasing, and is only limited now by the quantity of dry white amylose that can be made.

Amylose costs $3\frac{1}{2}$ to 4 cents a pound by wholesale. Until the price of corn became so high it was half a cent less than this. It is, therefore, a very profitable business to mix it with cane sugar and sell the whole for the same price which the cane sugar would fetch alone. I have here on the table specimens of these mixed sugars. Here are eleven samples made by the Manhattan Refinery, of New York, also six samples from the Atlantic Refinery, of Buffalo, and six samples from Henry Hobart, of New York. These sugars are sold retail under various names. Of these I may mention "New Process Sugar," "Niagara A B C," "Harlem B," "Excelsior C," and various others. To the eye these sugars look very much like straight cane sugars, and are generally pure and wholesome. They differ from the pure cane sugars in being less soluble in water and in being less sweet to the taste.

It has been estimated that amylose is two and a half times less sweet than sucrose; but this depends largely on the method of manufacture. Some samples of amylose will be found quite sweet, while others impart even a bitter taste.

In the manufacture of mixed sugars it is highly important that the amylose be dry. If hydrated amylose be used it is found almost impossible to pulverize it, and when ground it is pasty and sticky. Machines have been patented for obtaining finely granulated amylose from the well dried specimens. It is quite impracticable, however, to obtain amylose entirely dry, and it is capable of being worked very well when it still contains 8 to 10 per cent of water. This water is put in when sold at the same price as pure sucrose. In a commercial sense it is, therefore, not a disadvantage. The amylose which is used in mixing is generally made by high conversion under pressure. It, therefore, contains a high percentage of glucose, (dextrose) as compared with the maltose and dextrine present. It is, therefore, less sweet to the taste than the liquid amylose, where the percentage of maltose is larger.

Many schemes for the estimation of the different constituents of a mixed sugar have been proposed. For a discussion of the methods of analysis by reduction and fermentation, I refer to my paper read before this section last Saturday. I will content myself here with a brief outline of the method which I have employed. The water is estimated by heating two or three grammes in a flat platinum dish to 150° C. for two hours. The percentage of cane sugar I determine by Clerget's method. First get the total rotation in the polariscope then invert

* Read before the A. A. A. S., Cincinnati, 1881.

by heating to 68°, then polarizing again, carefully noting the temperature. From these readings the percentage of cane sugar present is calculated from the following formula :

$$x = \frac{a - a'}{144 - \frac{t}{2}}$$

Here a=first reading of polariscope.

a'=second reading of polariscope.

t=temperature of observation.

x=percentage cane sugar required.

In connection with the polariscope readings I also made reductions both before and after inversion, and thus obtained valuable data in regard to the nature of the amylose present, as well as securing a check on the optical results.

Following is a scheme of an analysis which will illustrate the method of procedure :

Reduction. Took 10g. in 1000 c.c. Of this, to reduce 10 c.c. Fehling's Solution, took 27.8 c.c.

Then 1000 : 27.8 = x : .05 (.05 g = sugar corresponding to 10 c.c. copper solution.)

Whence x = 1.8 g. = 18 per cent reducing matter.

Polarization. 26.048 g. in 100 c.c. gave..... 97°.8+
After inversion at 21° " 2°.-

Difference, = 100°.4

$100°.4 \div 144 - \frac{21}{2} = 75.2 = \text{per cent sucrose.}$

Reduction after inversion.

For 10 c.c. copper solution took 5.35 c.c.

Then 1000 : 5.35 = x : .05.

x = 9.35 g. = 93.50 per cent.

Deduct 18 per cent due to amylose = .. 75.50 "

Due to invert sugar..... "

Sucrose by polariscope..... 75.2 "

Amylose, water and ash by difference... 24.8 "

Following are the results of twelve examinations of mixed sugar :

No.	Per cent Reducing Matter.	Per cent Sucrose by Polariscope.	Amylose, Water, Ash, etc., by difference.
1.....	*29.70	*71.4	28.6
2.....	24.6	64.35	35.65
3.....	25.64	68.2	31.80
4.....	25.00	64.72	35.28
5.....	22.52	66.80	33.20
6.....	24.4	60.34	39.66
7.....	26.88	60.7	39.30
8.....	25.00	68.6	31.40
9.....	30.5	59.9	40.10
10.....	25.8	71.6	28.40
11.....	26.6	61.0	39.00
12.....	18.0	75.4	24.60

The analysis of mixed sugars is at this time a matter of great public interest. It is important that the public be not defrauded by purchasing sugars under false names. It is true that the manufacturers, as far as I know, do not sell the mixed sugars as *straight*, but when they pass into the hands of the retail dealers they are usually disposed of as if they were genuine. I do not anticipate that mixed sugars will jeopardize the public health. When well made they are certainly palatable and harmless. For boiling with fruits, etc., as in making preserves, they are quite as efficient as cane sugars. Nevertheless a "mixed sugar" should be bought, sold, and consumed as a mixed sugar, and thus all "winking" at fraud be prevented.

* One of these numbers is evidently incorrect. On looking over my notes I cannot find the mistake, and I have no more of the sugar with which to repeat the analysis. I think the error is in the per cent of reducing matter.—H. W. W.

COAL DUST AS AN ELEMENT OF DANGER IN MINING; SHOWN BY THE LATE EXPLOSION IN THE ALBION MINES IN NOVA SCOTIA.*

By H. C. HOVEY.

My object, in this communication, is to lay before the public by permission of Mr. Edward Gilpin, Inspector of Mines for Nova Scotia, the results of his investigations into the part played by coal dust in spreading and augmenting the terrible explosion that took place in the Albion mines, near Stellarton, on the East river, N. S., on the 12th of November, 1880.

Explosions are frequent in the coal mines of England and Belgium, causing loss of many lives and the destruction of much property. But in our own country, as a rule, we are fortunately exempt from such calamities. On the other side of the Atlantic there are special causes leading to these explosions; the thinness of the seams, the depth of the workings, the gaseous nature of the coals most prized for their coke and illuminating power, all combining to render difficult the great problem of producing ventilating currents sufficiently powerful and searching to insure the safety of the workmen.

Within the last few years men of scientific and practical knowledge have studied into these disasters and their causes, hoping that some remedy might be found that could remove the dreadful uncertainty hanging over the lives of those who help to sustain the fabric of modern civilisation. It was discovered by a rigid inspection of all available accounts of explosions, that many of them had occurred in pits known to be, as a rule, not dangerous from explosive gas, or declared to be free from it shortly before the moment of the accident. Then the fact gradually became apparent that a seemingly innocuous constituent of the mines, namely, *coal dust*, played an important part in spreading and augmenting the blasts. It was as if the wadding of a gun was composed of an inflammable material which, on ignition of the charge, doubled its effect.

It is well-known that chemical action is often induced in heaps of slack, such as exist in thick coal workings, and that the heat evolved may be enough to cause ignition. The danger is increased when the broken coal is comminuted, and floats in the air, as in this form, under various conditions, it may undergo rapid oxidation. Galloway's experiments show that when the particles of dust are so fine as to pass through the gauze shield of a safety lamp, an explosion may be caused by their ignition. Bauerman states that, in the Franco-Belgian collieries, "several fatal explosions have been traced to the firing of coal dust from the flame of a shot, even in cases where no fire-damp was present in the workings."

A brief glance at the history of the Albion Mines will not be misplaced. The main seam, which has been continuously worked since 1807, attains the remarkable thickness of 37 feet 6 inches, and is a highly bituminous coal, well adapted for gas and coke-making. It contains several layers of iron stone up to one foot in thickness, but may be considered as an unbroken mass of coal.

The earliest workings, now known as "the Burnt Mines," furnished large quantities of coal, until they were abandoned in 1839 on account of fire, which blazed so fiercely from the shafts that the chains used in raising the coal tubs, were melted. A new opening, the Bye Pit, was worked till 1863, when a fire occurred from a shot lighting gas. This was extinguished; but a short time after the gas ignited at the boilers, which were within a few yards of the top of the pit, and the works had to be closed up. The Foster Pit was next opened, but in 1869 spontaneous combustion caused a fire, which necessitated its abandonment before it was fairly under way.

* Read before the A. A. A. S., Cincinnati, 1881.

At length the Foord Pit was won out, and, with its great pumps and engines, formed one of the finest coal-mining establishments in America. The ventilation was effected by a large Guibal fan, similar to those used in the Pennsylvania Mines, and capable of circulating 120,000 cubic feet of air per minute through the ramifications of the mine. The workings of the Foord Pit, which is nearly 1,000 feet deep, extend about 1,800 yards to the north, and 1,700 yards to the south, having an average breadth of 250 yards. The galleries varied in height from 9 to 15 feet, being driven in the upper part of the seam, the lower part being left for later operations. The ventilation through the south side was maintained at the average rate of 25,000 cubic feet per minute. Shortly before the accident referred to, I went entirely through the colliery, in company with Mr. Gilpin and the overman, and we remarked the perfection of the ventilation, and the consequent absence of deleterious gases, even in the remotest bords.

On the morning of the disaster the night watchmen reported the mine to be free from gas, except in small and harmless quantities. From what source, then, originated the series of explosions that began within an hour from the time this report of entire safety was made, and continued at intervals until the mine became a furnace, whose flames could be subdued only by emptying into its burning chambers the waters of the adjacent river? Was there some sudden exudation of gas from the solid coal, or was this explosion due to the firing of coal dust from a safety-lamp or the flame of a blast?

None of the forty-four men who witnessed the beginning of the catastrophe escaped to tell the story or explain the mystery, and those rescued from more distant galleries had but conjectures to offer. The workmen on receiving the assurance that the mine was free from gas, received their orders, descended the drawing shaft, received their safety-lamps at the lamp cabin and a part of them went into the north side workings and the rest went into the south side dips and waited for their tools to be sent in for distribution. At this moment the explosion took place that was first noticed at the fan-shaft, where it blew the cover of the fan-drift off, and about one minute later it was apparent at the drawing shaft, having traveled in the one case *with* and in the other *against* the ventilating current.

The only additional facts definitely ascertained were gathered by an exploring party led by Mr. Gilpin, who, shortly after the original explosion and at the risk of life, descended into the pit and penetrated as far as the after-damp would allow them to go. The locality where the unfortunate workmen whom they tried to save were known to be was 1,200 yards south of the shaft; and the point reached by the party was only about 600 yards in that direction.

It was evident that the flame of the explosion had not reached as far as this, for there were no marks of fire on the dead bodies of men and horses found, nor was the splintered wood-work charred. They carried two corpses to the surface for examination, and it was found that one of these died of the after-damp, and the other from being dashed against some timber.

The walls of the galleries had been swept clear of timber, and presented the appearance of having been brushed with a broom. This was due to the passage of great volumes of dust which lay on the floor of the level in waves and drifts, into which the party often sank up to their knees. A similar effect was visible in the mine level, but not to so great a degree; as it was damper about the floor, and from the effects observed it would appear that, while the explosion passed along each level simultaneously, it had greater power in the lower one, as the doors were blown toward the upper, or main level. Clouds of the finer particles were carried up the shaft, and were swept on into the North-side levels.

At the lamp-cabin, where the safety lamps were

cleaned and given to the men after being examined by the shot-firers, an open light had been kept burning for years, as it was considered a safe place, being within a few feet of the bottom of the shaft. But here a *secondary* explosion took place, demolishing the cabin, burning the horses between the shaft and the cabin, and fatally injuring the lamp-man by igniting his oil-soaked clothing, so that he died in a few days. The effect of the explosion did not extend far into the north side, and some of the men there were ignorant of the disaster until warned by the over-man to leave the pit.

Secondary explosions caused by extracted, or generated gas, are nearly always in the vicinity of the primary one; but here is a case where the second was half a mile from the first, with an intervening space of at least a quarter of a mile known to be free from gas, because men were in it with lamps which showed no indications of its presence.

The ignition of these volumes of dust would, no doubt, have done serious injury to the shafting, had not the latter been wet and indeed saturated with water oozing under pressure through the upper strata into the shaft, and then falling to the bottom; so that, although elsewhere the mine was a very dry one, it was here in such a condition that the flame would be extinguished as soon as it touched the damp walls. The necessity of watering dusty mines has been pointed out by Inspector Gilpin, and this is said to be practised in some of the Belgian collieries. The present instance shows that such a precaution would tend to reduce the range of the explosion of the dust.

Attempts were made to restore the ventilation of the workings in the Albion Mines, when the presence of a large fire was discovered, and this made it necessary to flood the galleries. In about 48 hours after the explosion, a trench had been cut through to East River, and the water was let in at the rate of 15,000 gallons per minute, until, within a week, all the workings were filled. This, of course, made further investigations impossible, and nothing will be known beyond what has here been told.

The subject is one of acknowledged importance. There have been frequent explosions in flouring mills, said to be attributable to the ignition of flour dust. At a late meeting of the Manchester Geological Society, (in England), experiments were made to show that even finely powdered slate will spread the flame of gas explosions. Since the preparation of the present paper, a report has been made before the royal commission on accidents in mines, by Mr. F. A. Abel, Chemist to the British War Department, in which it is claimed, as demonstrated, that coal-dust is not only a fiercely burning agent, but when suspended in air currents may operate as an exploding agent. It operates, aside from its inflammability, as a finely divided solid, in "determining the ignition of only small proportions of fire-damp and air, and consequently in developing explosive effects," *i. e.*, under circumstances which, in the absence of the dust, would be attended by no danger.

HISTORY OF ALHAZEN'S PROBLEM.

Abstract of a paper read before the American Association for the Advancement of Science, Cincinnati, August, 1881, by MARCUS BAKER, U. S. COAST AND GEODETIC SURVEY, Washington, D. C.

Alhazen's problem is an optical one and was thus stated by the Arabian Alhazen for whom the problem is named. "*Given a luminous point and a point of vision unequally distant from the center of a convex spherical mirror, determine the point of reflexion.*" The solution of this problem involves the solution of the following geometrical problem now generally known among mathematicians as Alhazen's problem. *From two given points in the plane of a given circle draw lines meeting in the circumference and making equal angles with the tangent drawn at that point.*

This problem was first solved by Alhazen, a learned Arabian of the 11th century, and published at Basle, in Latin, in 1572. Since that time it has been studied by several distinguished mathematicians and a variety of solutions given. The paper presented contained a collection of these solutions aiming to be complete. Eleven solutions were contained in this collection, beginning with Alhazen and ending with a solution by E. B. Seitz, in 1881.

The first five solutions are by geometrical constructions, in which the points sought are determined by the intersections of a circle and hyperbola. The sixth solution, also a geometrical construction, is by means of the intersection of a circle and parabola. The seventh, eighth, ninth and eleventh solutions are by analytical or algebraical methods, while the tenth is a trigonometrical solution.

Among the people who have studied and solved the problem are Alhazen, Barrow, Hutton, Huyghens, Kaestner, Leybourn, L'Hospital, Robins, Seitz, Sluse and Wales. A complete list of bibliographical references was appended to the paper.

The paper contained further, an extension of the problem, *first*, to the surface of a sphere, and *second*, to an ellipse. The first case was illustrated by the following practical example:

The great circle track between San Francisco, Cal., and Yokohama, Japan, reaches nearly to latitude 52° N. The Pacific mail steamers plying between these ports usually avoid going north of latitude 45° N. Now, if the 45th parallel of latitude be designated as one north of which the steamer is not to go, in what longitude must this parallel be reached in order that the steamers' path between the ports shall be the shortest possible? The extension of Alhazen's problem to the surface of the sphere solves this problem and the longitude required is 168° W. from Greenwich.

The extension of the problem to the case in which an ellipse replaces the circle gives rise to a very complex equation of no special value.

WASHINGTON, D. C., Sept. 13, 1881.

ROTATION OF REDUCING POWER, AS MEASURED BY FEHLING'S SOLUTION TO THE ROTATORY POWER OF COMMERCIAL AMYLOSE (GLUCOSE AND GRAPE SUGAR).

SECOND PAPER.*

BY PROF. H. W. WILEY, Lafayette, Ind.

In a paper read at the Boston meeting of this Association¹ I called attention to the fact that the reducing power of Amylose, measured by Fehling's Sol., could be readily determined by the polariscope. Since that time I have extended the series of observation then reported, and with such results as justify the conclusions at which I arrived.

In commercial Amyloses, whose specific gravities do not vary much from 1.410, the reducing power is reliably calculated from the reading of the polariscope. The average percentage of water in these Amyloses is nearly thirteen. If we allow one per cent for optically inactive substances present, we may safely place the optically active matter at 86 per cent. By prolonged boiling with acids, even if they be quite concentrated, only about 82 per cent of reducing matter is obtained.² Further boiling causes the mass to turn brown, and may even cause a decrease in the amount of reducing matter found. Since there is so much difference of opinion respecting the reliability of Fehling's solution, and since

there is no other reducing mixture that works as well, it would, perhaps, be better to use the polariscope for the determination of the amount of substances present in an Amylose capable of reducing the various solutions used for grape sugar measurements.

In the following table the calculation of the reducing power was made by the formulæ,³ which I have already explained. Although, in a few cases, the specific gravity varied by a few thousandths from 1.410, the difference has not been of sufficient importance to make any correction⁴.

Since the ordinary Amyloses, called grape sugars, of commerce differ from those called glucoses only in having the processes of conversion carried further, it is found that the same rule applies to them also. In fact, I believe it will be found true with all varieties of Amylose made by use of sulphuric acid, provided 8.6 grammes of the anhydrous substance be used in each 100 c.c. of the mixture to be examined.

Following are the results of my observations:

TABLE I.

No.	Sp. gravity.	Reducing matter by Fehling's Solution.	Rotation of 10 g. in 100 c. c. cone sugar scale.	Reducing matter calculated from Polariscope.	Difference.		Date of Manufacture.
					+	-	
1	1.414	52.1	53.04	52.05	0.04	...	1880. September 15
2	1.419	52.2	53.00	53.00	0.08	...	" 14
3	1.410	53.8	51.00	55.05	1.07	...	" 15
4		53.2	55.05	49.00	...	3.3	October 12
5	1.412	51.0	54.01	51.06	0.06	...	" 18
6	1.413	51.1	53.02	52.75	1.65	...	" 19
7	1.417	51.6	53.45	52.44	0.84	...	" 19
8	1.417	49.7	55.02	50.03	0.6	...	" 20
9	1.408	49.0	55.05	49.09	0.09	...	" 21
10	1.413	49.5	55.04	50.00	0.05	...	" 21
11	1.411	48.1	56.06	48.05	0.04	...	" 17
12	1.421	48.8	56.04	48.08	0.00	0.0	" 16
13	1.417	50.0	57.00	48.00	...	2.0	" 16
14	1.413	46.4	56.07	48.04	2.00	...	" 14
15	1.417	48.1	56.05	48.06	0.05	...	" 14
16	1.418	46.3	58.02	46.05	0.02	...	" 13
17	1.412	47.2	57.00	48.00	0.08	...	" 12
18		72.0	37.03	72.63	0.63	...	Unknown.

The above analyses were of samples sent by the manufacturers, the Peoria Grape Sugar Company. They represent the whole number of samples examined and in the order in which the analyses were made. Seventeen of them were of syrups, and the eighteenth of a solid sugar. Only four out of the eighteen show discordant results. In one of these the specific gravity was not determined. It was my intention to make these four analyses in duplicate, but a press of other business prevented. In general, it appears that the results given by the polariscope, by the above method of calculation, are a little too high. If they were diminished by 5 the agreement would be better. That the reducing power of Amylose can be correctly calculated from its rotatory power is certainly established from the thirty-eight unselected instances which have been presented.

ELECTRIC LIGHT FOR LIGHTHOUSES—The first of the series of lighthouses round the French coast which are to be henceforth illuminated by electricity, has, with all its necessary machinery, been completed. It is called the "Phare de Planier," and is situated at the mouth of the Rhone, near Marseilles.

* Read before A. A. A. S., Cincinnati, 1881.

¹ Proceedings of this Association, 1880, p. 308; Journal Am. Chem. Soc., Vol. II., p. 387.

² Proceedings A. A. A. S., 1880, p. 320. Journal Am. Chem. Soc., Vol. II., p. 399.

³ Proceedings A. A. A. S., 1880, p. 313. Journal Am. Chem. Soc., Vol. II., p. 393.

⁴ Proceedings A. A. A. S., 1880, p. 316. Journal Am. Chem. Soc., Vol. II., p. 395.

ON THE INTERIOR CONDITION OF THE TERRESTRIAL GLOBE.

BY M. E. ROCHE.

Generally we admit that the earth is entirely fluid in its interior, with the exception of a thin crust, and most of the mathematical studies on the figure and the constitution of the earth assume this fluidity. Thus by attributing to this fluid a certain law of compression, Laplace has deduced a corresponding law of densities, which Legendre had already examined before him, and which permits the calculation of the flattening of the different surfaces *de niveau* of the terrestrial mass. I have myself proposed another law of compressibility, which conducts to a very simple formula for the increment of the density. The conditions which every hypothesis must satisfy, on the distribution of the mass in the interior of the earth, are that it must accord with the value of the superficial flattening, and also with a certain constant depending on the phenomenon of precession. These conditions are very approximately satisfied in the hypothesis of fluidity, if we admit that the terrestrial flattening is nearly $\frac{1}{230}$; but if this flattening is greater than $\frac{1}{235}$, as it seems to result from the most recent determinations, the agreement no longer exists.

There is need, therefore, of a new examination of these researches under a different hypothesis, for example, in considering the globe as formed of a nucleus or solid mass very nearly homogeneous, covered with a lighter shell whose density, from geological considerations, can be estimated as $\frac{1}{3}$ with respect to water. This constitution of the globe being supposed, I find that it is possible to conciliate the actual values of the precession and of the flattening, if we take account of this, that the interior nucleus of the globe solidified and has taken its definitive form under the influence of a rotation less rapid than that with which the earth is now animated.

In every case the contraction due to this cooling of the globe must lead to a progressive acceleration of its angular velocity. But if this globe is fluid the figure of the different strata adapts itself continually to the rotation which has place at each instant, in such a way that finally there remains no trace of the successive variations which their flattening have undergone since the origin. If, on the contrary, at a certain epoch of the cooling the interior strata have passed to a solid state, these strata have taken and preserved a flattening very different from that which would be attributed to them by the general equation of hydrostatics applied to a mass entirely fluid and possessing a rotation common to all its parts. The formulæ calculated in the hypothesis of a solid nucleus contains at the same time the constant g , the actual ratio of the centrifugal force to the equatorial gravity, and value g_0 , of the same ratio at the epoch of the solidification of the central mass. This last element, not being determined, we can give to it a value such that the superficial flattening accords with the coefficient of precession. It is necessary for this to suppose g_0 less than g , whence it results that the terrestrial rotation has undergone an acceleration since the consolidation of the interior nucleus.

The physical and astronomical conditions of the problem permit also the determination, with some precision, of the dimensions and specific weight of this central mass. If we leave out of consideration the crust purely superficial, as also a slight condensation towards the centre where the heavier materials would be collected, the constitution of the globe will be as follows: a nucleus, of which the density is nearly 7, covered with a shell of density 3, whose thickness does not attain one-sixth of the entire radius.

The central terrestrial mass is therefore in specific weight analogous to meteoric iron, while the stratum that envelops it is comparable to aerolites of a stony nature, where iron enters only in a small proportion.

BOOKS RECEIVED.

THE MICROSCOPE AND ITS RELATION TO MEDICINE AND PHARMACY. Edited by CHAS. H. STOWELL, M. D., and LOUISA REED STOWELL. Published monthly. Ann Arbor, Michigan, 1881.

There are already two journals published monthly which are devoted to microscopy, it is therefore with some surprise that we find a third journal of the same description appealing to the patronage of Microscopists.

"*The Microscope*" claims to supply a want in offering physicians a journal which treats exclusively of Medical and Pharmaceutical Microscopy, thus differing from the two former microscopical journals which cover the whole field of Microscopy.

We believe that the success of "*The Microscope*" will depend upon that journal being conducted strictly within the limits of its own programme. Undoubtedly the majority of American microscopists are members of the medical profession and, therefore, "*The Microscope*" may look for a numerous constituency.

"*The Microscope*" has been produced in an excellent form, is well printed, and illustrated with good illustrations, and if the editors will confine the columns to *Microscopy*, to the exclusion of facetious "items" clipped from their exchanges, they may hope to place their journal on a firm basis.

CORRESPONDENCE.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE."

Permit me to suggest a few questions that should be answered by the author of "The Great Primordial Force." (See "SCIENCE," p. 405.)

Let me introduce my questions by announcing my *belief* in the conservation of energy and in the unity of force and by expressing a *hope* that the time will come when the phenomena peculiar to the different *manifestations* of force may be comprehended and their identity demonstrated.

The following questions are respectfully suggested:

1. When, and by whom, has it been demonstrated that gravitation is resolvable into light or heat?
2. If all force "is substance," or matter, (see "SCIENCE," p. 405, last paragraph), then, at least some matter is force. What then is the distinction between matter and force?
3. On p. 406, it is asserted that the two elements, "*Motion and Magnetism*," "develop all known forces of the universe." How can motion exist without a *prior* force, and what *is* magnetism?
4. What produces the revolution of the revolving celestial armatures?

The foregoing questions may be sufficient for the present, but it is due to science that statements claiming to be scientific should be *truths*. Let us examine some of the author's statements.

It is asserted that "if gravity acts inversely as the square of the distance, then the earth at aphelion could not, without the aid of some other force, return to perihelion." It is to be feared that the author of the above question is not aware that the inertia of matter is an experimental fact, and is entirely sufficient to bring back the planet from aphelion to perihelion.

As to the law of inertia, all matter, if not acted upon by some external force, will continue in its present state, whether of motion or rest, and because while a planet is passing from perihelion to aphelion the tangent to its orbit makes an obtuse angle with its radius vector; therefore its inertia counteracts, and to a certain extent,

opposes its centripetal force, from perihelion to aphelion, and because at aphelion, the tangent of its orbit, is at right angles with its radius vector, and from aphelion to perihelion the angle between the tangent and radius vector is acute, therefore, the inertia of the planet co-operates with the centrifugal force from aphelion to perihelion.

Hence, *inertia*, in conjunction with the centripetal force, is sufficient in all cases of elliptic motion, to bring back the planet from aphelion to perihelion.

Again, the writer says, (p. 407) "the degree of ellipticity of each planetary orbit is due to the inclination of its axis." That the reader may judge how much weight should be allowed the author's statement, let this statement be compared with known facts.

The eccentricity of the earth's orbit is .0167922; that of Venus is .0068618. The inclination of the earth's axis is about $23\frac{1}{2}^{\circ}$; that of Venus 75° . That is, the inclination of the earth's axis to its orbit is less than one-third that of Venus; whereas, the eccentricity of the earth's orbit is more than twice that of Venus.

The author's statement, therefore, is not only not supported by the facts, but is in conflict with the facts.

J. E. HENDRICKS.

DES MOINES, Sept. 3, 1881.

To the Editor of "SCIENCE":—

The prevailing scientific ideas of any period are regarded by the common people, and even by the scientists of that period as indubitable facts, without due examination of their origin or their foundation. But further thought and observation often compel a reluctant retreat therefrom. Thus present conceptions have all the weight of perfect truth. We call them "*facts*."

The rapid discoveries of the present age, and the unprecedented freedom of thought are disturbing all theories that are not well founded. The new law of Conservation of Force will cause the final destruction of every theory that is not in harmony with it. The paper upon the "Great Primordial Force" was but an effort to bring the explanation of the physical forces into consistency with that all-governing law.

That the paper should give rise to such questions as those proposed by our critic was most natural, and we shall endeavor to answer them in the same candid spirit in which they were asked.

1. Bodies fall by force of gravitation. Resistance to such fall of course produces light and heat, precisely as resistance to the motion of the electrical current produces the same. If we admit that the electrical current is convertible into these forms, by parity of reasoning, so is gravity.

2. The relations of matter and force seem adequately set forth in the paragraph referred to. (See "SCIENCE," p. 405.) Electricity, which in its tenuity pervades all matter, is abundantly demonstrated to be itself matter in varied form, as in the thunderbolt, the fire ball, and in the St. Elnios fire.

3 and 4. The only rational explanation, whether to us satisfactory or unsatisfactory, of the origin of force, is found in the hypothesis and admission of already existing force, the *primum mobile*. HELMHOLTZ says, that a body set in motion around the sun in vacuous space, and with a certain velocity will continue to move with the same velocity to all eternity. It is sufficient for us to know that the motion *is*, and the magnetism *is*, and thus we have the "celestial armatures" already in revolution,—the effects of which it is for us to observe. To us the effects are, and are called, the "physical forces."

Our critic is disturbed by our questioning of the dogma that "gravity acts inversely as the square of the distance,"—on the ground that if that force is weakened by the earth's being removed to aphelion, it could not again bring back the body to perihelion. We re-affirm FARADAY'S position; "The received idea of gravity appears

to me to ignore entirely the principle of the conservation of force, and by the terms of its definition, if taken in an absolute sense, 'varying inversely as the square of the distance' to be in direct opposition to it." But we would not rest the assertion upon any great name. It is evident that inertia can "bring back" nothing, that inertia, or momentum, or centrifugal force, or whatever other expression is used, may effect only motion in a straight line. Momentum, (which evidently is what the critic means by "inertia,") has no tendency towards circular motion. It is attraction, gravity, centripetal force alone that draws, or "brings back," and if that force is *weakened*, how can it make itself stronger? If once diminished (as the principle, "gravity acts inversely as the square of the distance" necessitates,) then the opposite force has the balance of power, has destroyed the equilibrium, and except some favoring force steps in to restore the lost ground, momentum, ("inertia") must forever send it farther and farther into space.

Finally, our critic in order to show that the electrical theory, which makes the inclination of a planet's axis to govern the ellipticity of its orbit, is not true to fact, adduces the instance of Venus. This asserted "*fact*" (?) that Venus' axis has an inclination of 75° , is wholly unfounded, Astronomers to-day are not so ready to assert it. The dazzling brilliancy of this planet prevents any positive disclosures as to the period of its daily revolution, to say nothing of the more delicate and difficult determination of the inclination of its axis.

Our distinguished astronomer NEWCOMB says:—The latest physical observations on Venus, with which I am acquainted are those of Dr. VOGEL, "Bothkamp Observations, 1873." The result to which these observations point is that the atmosphere of Venus is filled with clouds so dense that the solid portion of the planet cannot be seen, and no time of rotation can be determined." HERSCHEL said that he was never able to see any permanent markings on Venus,—but it is only by such markings that these determinations are made.

H. RAYMOND ROGERS, M. D.

DUNKIRK, N. Y.

MEDICAL CONGRESS NOTES.

(London, 1881.)

At the close of Professor Huxley's address, Mr. MacCormac followed with a statement, the most important items of which were that the number of members amounted to 3,210; that the sections had held 11 meetings, extending over 293 hours; that there had been delivered 464 written papers and 360 oral addresses. The attendance at the sections had been large, and had not shown signs of falling off even quite to the close. The museum was referred to as a great success, and the demonstrations of living patients had been attended by crowds each morning.

ENDORISING VIVISECTION.

Sir James Paget then presented the following resolution forwarded by Professor M. Foster, from the Physiological Section: "That this Congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable for its future progress; that, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion that, in the interest of men and animals, it is not desirable to restrict competent persons in the performance of such experiments." Pointing out that it was impossible to discuss such a resolution then, the President asked those who were opposed to it to record their names and votes at the close of the meeting. The resolution was then adopted with loud cheers, and no hand was held up in opposition to it.

Director Meteorological Observatory of the Department of Public Parks, New York.